THURSDAY, JANUARY 8, 1891.

THE SOUTH KENSINGTON AND PADDINGTON SUBWAY RAILWAY.

WE print elsewhere a letter from General Webber, in which he begins by stating that the land at South Kensington which is under the control of the Exhibition Commissioners, was originally, to use the words of the late Prince Consort,

> "At no distant date to form the inner court of a vast quadrangle of public buildings, rendered easily accessible by the broad roads which surround them, buildings where science and art may find space for development, with the air and light which are elsewhere well nigh banished from this overgrown Metropolis."

If General Webber had been merely a company-promoter he would never have supplied us with so apt a quotation, telling so dead as it does against his underground railway; but the fact is, his appreciation for science has unconsciously led him to give the scientific rather than the Stock Exchange view of the matter. By all means, we urge, let the destiny foreshadowed by the late Prince Consort be fulfilled, especially as regards the "buildings where science may find space for development."

Again, General Webber's sincerity compels him to be quite silent about the disastrous disturbance that his railway, if ever constructed along the route now proposed, would cause to the pursuit of scientific investigation in the various present laboratories in Exhibition Road. The only remedy he can suggest is that "laboratories and observatories for original research used for the instruction of a very few experts may have to find a home elsewhere." Now, even this remedy he would never have advanced had he realized that his "very few experts" are in actual fact some hundreds of students, all of whom are now daily using the very apparatus, and making the very experiments, that his railway would render impossible. And since many of these students are being trained to become teachers, the education now given to hundreds daily in Exhibition Road is indirectly the teaching of thousands throughout Great Britain.

But we will go farther and say that, even if the interests of large numbers of present and future students were not at stake, and even if General Webber's contention were correct, that the work of only a few experts would be stopped by his railway, that alone should compel the railway to seek another route. For the work of these w experts at South Kensington, some of whom are pidly advancing pure science, and others of whom e as rapidly advancing applied science, will produce a r wider effect on the future of this country than the utilization or non-utilization of the existing subway by the South Kensington and Paddington Railway Syndicate.

Towards the end of his letter General Webber feels bound, however reluctantly, to act the true "company-promoter," and so he refers to "the steady annual decline in the records of the numbers of visitors to the permanent institutions," as an argument to show the necessity of a new mode of locomotion. Probably this reference to "the steady annual decline" is only a quotation from the prospectus of the "South Kensington and Paddington Subway Company," and so ought to be taken with that liberal seasoning of salt appropriate to prospectuses of new companies. For, as a matter of fact, "the steady annual decline in the records of the numbers of visitors" is disposed of at once on examining the records themselves, which give the following figures:—

Number of visitors to the South Kensington Museum, the Natural History Museum, the Indian Museum, and the Museum of Scientific Apparatus.

During	1886	1,206,741
,,	1887	1,146,590
,,	1888	1,270,027
,,	1889	1,235,854
,,	1890	1,187,142

from which we see that the numbers during the last two years together were actually larger than the numbers during the first two together.

General Webber says that by his proposed railway "easy and rapid communication will also be afforded to hundreds of thousands of persons living to the north of Hyde Park to visit and use all the great institutions collected at South Kensington." Would not the communication be just as easy and just as rapid if the railway went down Queen's Gate, and connected the Gloucester Road Station on the District Railway with Paddington? For the main entrances to the Albert Hall, the Natural History Museum, and the Imperial Institute, are just as near Queen's Gate as Exhibition Road. The South Kensington Museum is no doubt on the Exhibition Road side of the space, but to balance this the Exhibition of Machinery and Inventions is on the Queen's Gate side of the area in question.

The public will be equally well served along whichever of the two alternative sides of the Albert Hall the railway goes. But, while an untimely selection of the eastern, or Exhibition Road, route would mean a disastrous stoppage of the scientific work now being carried on, the trains might run along the western, or Queen's Gate, route without causing practically any magnetic or mechanical disturbance to the delicate instruments in use at the existing laboratories.

We shall probably be answered that, unless the present subway (the length of which, as a matter of fact, is but one-tenth of the whole length of the proposed railway) be utilized, the District Railway and General Webber's syndicate will not make quite so much money out of the scheme. Possibly not; but is the London home of scientific research to be destroyed merely to enable General Webber's syndicate to pay extra dividends?

"All attempts hitherto made to complete the work" (that is, the subway) "as far as the Albert Hall have failed," says General Webber. May his present scheme, the ill-omened child of many failures, say we, walk in the footsteps of its fathers.

ARE THE EFFECTS OF USE AND DISUSE INHERITED?

Are the Effects of Use and Disuse Inherited? An Examination of the View held by Spencer and Darwin. By William Platt Ball. (London: Macmillan and Co., 1890.)

THE question which constitutes the title of this essay still continues—and is likely for some time to continue—the most important question in the field of

Darwinian thought. On the one side we have the school of Weismann, which answers the question with an unequivocal negative. On the other side we have the writings of Darwin himself, which entertain the so-called Lamarckian factors as subsidiary to natural selection, or as lending considerable aid to natural selection in carrying out the work of adaptive evolution. Again, we have the writings of Herbert Spencer, which attribute a still higher proportional value to the Lamarckian factors; while, lastly, we have the self-styled neo-Lamarckians, who regard these factors as of even more importance than natural selection. Amid so great a conflict of opinions on a matter of such extreme importance, any survey of the actual evidences in favour of the Lamarckian factors cannot fail to be opportune, even though the value of such an attempt must depend upon the care and the judgment with which it is undertaken. Now, we are glad to say that the essay before us is, in all respects, as admirable as it is opportune. Scientific in spirit, and logical in execution, it deals with its subject in a manner at once concise and exhaustive. Restricting his ground for the most part to the domain of fact, Mr. Ball has made a full inventory of the cases, or classes of cases, which have hitherto been adduced in evidence of the transmission of acquired characters, and briefly weighs the value of the evidence in each. In the result he concludes that there is no real evidence for any of the cases; and as his work is throughout performed in a thoughtful and painstaking manner, we deem it the most instructive contribution which has hitherto appeared upon the subject of which it treats. Of course the writings of Galton and Weismann present the greater merit of having been the first publicly to challenge the doctrines of Lamarck; but this they did on grounds of general reasoning, and by viewing the evidences of those doctrines, as it were, *en masse*. The merit of Mr. Ball's work, on the other hand, consists in its detailed analysis of each of the facts and arguments which have ever been brought forward in support of what he conveniently calls "use-inheritance."[1] He thus restricts himself to the one question of fact, whether or not there is any good evidence of the transmission of acquired characters, without embarking upon any general theory of heredity. And, as already remarked, he has done this purely analytical work in an exceedingly able manner. So much, indeed, is this the case, that we can find but little to say in the way of criticism; and that little must take the form of pointing out the particular cases where it seems to us that his examination is not quite so thorough as it usually is.

He begins by taking *seriatim* "Spencer's examples and arguments," and the first of these is "diminution of the jaws in civilized races." Here he shows that "cessation of the process by which natural selection favoured strong thick bones during ages of brutal violence might bring about a change in this direction;" and he points to the simultaneous thinning of the skull, &c., as virtual proof that such is the true explanation. Nevertheless, in the next section, which treats of "diminished biting muscles of lap-dogs," he invokes the principle of artificial selection to explain the facts, thus: "The conscious or unconscious selection of lap-dogs with the least tendency to bite, would easily bring about a general enfeeblement of the whole biting apparatus—weakness of the parts concerned favouring harmlessness" (pp. 12, 13). But surely, if the cessation of selection is sufficient to account for the diminution of the biting apparatus "in civilized races of mankind," it is no less capable of explaining a similar diminution in the case of "lap-dogs." If artificial selection has taken any part in the matter at all, it must have done so in the direction of "favouring harmlessness" by acting on instincts or dispositions rather than on jaws and muscles (see NATURE, vol. xxxvi. p. 405).

In opposing Spencer's argument drawn from the facts of co-adaptation, Mr. Ball appears somewhat unduly to assume the character of a special pleader. The argument is, that the more the utility of any co-ordinated set of parts depends upon their co-ordination, the more difficult does it become to see how natural selection alone could have produced the mechanism. For if the mechanism be such that its utility depends on all its parts simultaneously co-operating, no utility can arise unless all the parts are developed simultaneously in the same individuals. Now, Spencer represents that the chances must be enormously against an accidental occurrence of many concomitant changes in any single individual, where such changes are thus without benefit to the individual save when they do occur in combination; while, of course, the inherited effects of use and of disuse of all the parts concerned would explain their simultaneous evolution. Mr. Darwin answered this argument by showing how natural selection might be held to operate in the production of these effects; but in doing so he continued to attribute a probably large share of the work to the Lamarckian (*i.e.* Spencerian) factors. Now, Mr. Ball, in quoting Darwin's opinion, ignores this latter point. For instance, after giving it as Darwin's view "that natural selection alone 'would have sufficed for the production of this remarkable quadruped'" (*i.e.* the giraffe), he omits the conclusion of Darwin's sentence, viz. "but the prolonged use of all the parts together with inheritance will have aided in an important manner in their co-ordination." Again, while referring to what Darwin has said touching another of Spencer's examples—the elk—Mr. Ball omits to notice the sentence: "Although natural selection would thus tend to give to the male elk its present structure, yet it is probable that the inherited effects of use, and of the mutual action of part on part, have been equally or more important."

In bringing together all the cases adduced by Darwin to support the theory of use-inheritance, Mr. Ball begins by observing that

> "he [Darwin] appears to have acquired the belief in early life, without first questioning and rigorously testing it, as he would have done had it originated with himself. In later life it appeared to assist his theory of evolution in minor points, and in particular it appeared absolutely indispensable to him as the *only* explanation of the diminution of disused parts in cases where, as in domesticated animals, economy of growth seemed to be practically powerless. He failed to adequately notice the effect of panmixia, or the withdrawal of selection, in causing or allowing degeneracy and dwindling under disuse."

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[1] This term is coined by him as equivalent to Darwin's "inherited effects of use and disuse," to Spencer's "inheritance of functionally produced modifications," to Weismann's "inheritance of somatogenetic characters," &c.

These remarks are very judicious, as likewise are those where, further on, he alludes to the important distinction between selection as merely withdrawn (panmixia) and as actively "reversed" (through economy of growth, &c.). For it is shown that, if Darwin had not thus failed adequately to notice the former principle without reference to the latter, he could not have continued to regard our domesticated animals as furnishing any conclusive proof of use-inheritance; or, indeed, any better evidence than is furnished by animals in a state of nature. Having satisfied himself that there can be no economy of growth in our highly-fed domesticated animals, seeing that parts may reappear in them which are obsolete in their parent or undomesticated types, he concluded that, where other parts presented diminution, the fact could not be attributed to a reversal of selection. Therefore he attributed it to the inherited effects of disuse as to the only imaginable alternative. But, as Mr. Ball—following Weismann and others—plainly shows, the mere cessation of selection, without any reversal of selection, is in all such cases bound to produce the effects observed; with the consequence that the former principle "invalidates Darwin's strongest evidence for use-inheritance."

Passing from such general remarks on Darwin's attitude of mind with respect to the question of use-inheritance, Mr. Ball deals separately and exhaustively with all the particular instances that Darwin has given. The criticism here is uniformly good, and as regards some of the cases—*e.g.* "wings and legs of ducks and fowls"——remarkably so. But as we have no space to consider these numerous instances in detail, it must be enough to say, in general terms, that Mr. Ball has certainly reduced their evidential value to very small dimensions. Even the results of Brown-Séquard's experiments on the apparent transmission of injuries are shown to be of a more doubtful character in relation to the question of use-inheritance than anybody else has hitherto indicated.

There are, however, two or three remarks which seem worth making on Mr. Ball's treatment of some of Darwin's examples. Thus, in considering the "larger hands of labourers' infants" as compared with those of infants belonging to the upper classes, he attributes the phenomena to "sexual selection in the gentry." And in many other cases, both in man and the lower animals, he shows how the apparent effects of use-inheritance may be due to this cause. Such, of course, is a very reasonable position for Mr. Ball to adopt; but what is to be said about all such cases by the school of Wallace, which rejects the theory of sexual selection as well as that of use-inheritance? These cases, of course, are cases where the theory of natural selection cannot be applied; and therefore it would seem that the school of Wallace must either confess them inexplicable, or else devise some additional theory for the purpose of explaining them.

This allusion to the views of Mr. Wallace leads to a consideration of an important argument recently published in his "Darwinism"—namely, that, even if use-inheritance be physiologically possible, it can never be allowed to act, inasmuch as natural selection will always effect the required alterations more rapidly than they could be effected by use-inheritance. The only answer to this argument appears to be, as Mr. Ball puts it, "that slight changes in each generation need not necessarily be matters of life and death to the individual, although their cumulative development by use-inheritance might eventually become of much service." This answer he disposes of by adding: "But selection would favour spontaneous variations of a similarly serviceable character." This, however, does not appear to meet the requirements of the case. For the whole point of the answer to Mr. Wallace's argument lies in the consideration that "slight changes in each generation need not necessarily be matters of life and death to the individual," *i.e.* that the amount of change due to use-inheritance may be so small *in each successive generation* as not to make any appreciable difference in the struggle for life. Hence this amount of change need not be in any degree "serviceable," although, by an accumulation of such changes in the same line of change, a high degree of serviceability may be attained without the necessary aid of selection. Of course, this answer to Mr. Wallace's argument can only go upon the supposition on which the argument itself is founded—namely, that use-inheritance is physiologically possible; and therefore both the argument and the answer are irrelevant to the question of such possibility as raised by Weismann.

Once more, in the same section of this essay—namely, that which is concerned with Darwin's examples of use-inheritance—Mr. Ball repeatedly adduces the case of neuter insects as demonstrative evidence against use-inheritance. But he does not consider the possibility of the instincts of neuters being survivals from a time when all the female insects of a hive were both fertile and industrious. He quotes, indeed, an absurd passage from Büchner upon this subject, but does not allude to what Perrier has said. We are far from maintaining that Perrier has made out his case; but we think that the question raised by him ought to have been gone into by Mr. Ball. Moreover, in the event of there being a second edition of his essay, it is to be hoped that Mr. Ball will consider the ingenious suggestion with regard to these instincts which has just been published by Prof. Lloyd Morgan in his admirable treatise on "Animal Life and Intelligence," pp. 440-42.

The concluding part of the treatise is devoted to "Miscellaneous Considerations." These all appear to us both apposite and cogent, except the sections which argue that use-inheritance, even if it were physiologically possible, would prove of more harm than good in the matter of adaptive evolution. In the first place, there was no necessity for Mr. Ball to propound such a question—his aim elsewhere being to show that, as a matter of fact, there is no evidence of use-inheritance having ever been in operation. In the next place, this foreign and superfluous argument is not well sustained; for although Mr. Ball shows that in many instances use-inheritance would be an "evil," he not only disregards the vastly greater number of cases in which it would helpfully co-operate with natural selection, but he also disregards the important consideration that in all cases the "minor factor" would require to be under control of the "major factor"—with the result that, where harmful, its effects would not be allowed to develop.

Although we have thus devoted considerable space to a review of this little book, we regret our inability to devote more. For, having restricted ourselves to points where criticism seems possible, we must have failed to give a sufficient idea of all the rest of the essay. In conclusion, then, we must add that Mr. Ball's analysis as a whole appears to us to stagger the theory of use-inheritance more seriously than ever it has been staggered before; and, therefore, that no one who henceforth writes upon the subject can afford to disregard his treatment of the question, "Are the Effects of Use and Disuse Inherited?" GEORGE J. ROMANES.

TECHNICAL EDUCATION.

Manual Training in Education. By C. M. Woodward, A.B., Ph.D. (W.U.), &c. With Illustrations. (London: Walter Scott, 1890.)

THIS is in some respects a valuable work, and therefore it is the more to be regretted that its title is in a double sense misleading. A very little reflection would have taught the author that "manual," as from *manus*, the hand, means anything which hands are capable of effecting. He, however, defines "manual training" as limited to teaching and learning the use of tools and working materials; these, according to his system, being limited in turn to wood and metal work, such as turning, carpentry, and smithing. The immense range of work of which children are capable is not included, therefore, in "Manual Training." Again, we find in the book that the education in question not only does not include that of girls, but actually takes no note of young children of either sex. "Starting with boys in their teens," Prof. Woodward shows, what has always been well known, that such boys can be taught the rudiments of certain "trades." But as the very great majority of children leave school early in their teens to go into active life, what parents or the public chiefly wish to know is, what manual training can be imparted to all children while they are yet at school? Extensive experiment has perfectly shown that they can be taught to draw, model, and execute much useful art work even from six years of age, and what is of more importance is that, as one hour of sleep before midnight is worth two after it, so those who learn to draw in early childhood acquire a certain dexterity and skill such as is rarely, if ever, attained after thirteen years of age. "The proper mental maturity," says Prof. Woodward, "rarely comes before the fourteenth year. I think of the class as about fifteen years old." "The minimum rate of admission" (to my school) "is fourteen years"—meaning, we suppose, that no younger pupils are received. If this means anything, it is that, according to the author, manual training in education should not begin till "the proper mental maturity" is attained. But what we expect from education is that pupils shall be trained *before* their minds are matured.

There are many persons who will buy this work under the impression that it will teach all the details of manual training. But, in fact, of its 310 pages only 77 are devoted to practical instruction in drawing, wood and iron work. These are truly excellent of their kind, so much so that we cheerfully wish *si sic omnes*—"would that all were like it"—since in that case we should have had a work of practical use, although there is at present no lack of admirable if not better hand-books for such training in England. In the remaining part of the book Prof. Woodward advances theories which no man of culture and intelligence can admit, and which imperatively call for refutation, since at present the British public in its bewilderment as to "technical education" is being extensively deluded by them.

It has been said by some journalist that the aim of the Socialists seems to be to make of all society a well-organized poor-house. It is not in the least an exaggeration to say that Prof. Woodward's idea of education is that every male in a community shall be, first of all, a mechanic. He admits, it is true, a moderate amount of culture in literature and other branches, but exacts that *three hours a day* shall be given to drawing and manual labour, while with boys above fourteen it may be more. That is to say, three hours in school, with two hours of home study, are to be given to mathematics and book-keeping, science (*i.e.* geography, zoology, botany, chemistry, physics, physiology) and literature (which is to include "some choice specimens of modern prose and poetry"), and *one* foreign language—French, German, or Latin. But it is very evident that Prof. Woodward considers that in all cases, with every pupil, the manual labour should be the most thoroughly taught, and that carpentry and metal work are of paramount importance. The main force or tendency of all education should be to form a mechanic, and give to every youthful mind the habit of regarding all things from a mechanic's point of view.

Now, while this is a commendable education for a blacksmith, and while we may admit that it amuses and pleases youth, it is evident "on the face of it" that the literary course prescribed is utterly inadequate to *properly* prepare pupils for any career above that of the workshop. And this tone pervades the whole book: "there's nothing like leather" appears on its every page. The author quite forgets, what is also left out of sight by most of our own reformers in education, that, absolutely necessary as it may be to educate the majority to become mechanics, the world requires a very respectable number of professional, literary, and really scientific men, who could not be properly trained for such pursuits on one language, even with a knowledge of "some choice specimens of *modern* prose and poetry."

It is true that the author admits that, when "a lack of mechanical interest or power" manifests itself, the lad should unquestionably be sent to his grammar and dictionary rather than to the laboratory and drafting-room." That is to say, when the bit of wood is fit for nothing else we may make a god of it. And a master mechanic is to decide as to who shall thus take the back seats in education, and occupy the inferior positions of men of science and literature!

There is a class of boys, according to Prof. Woodward, "who are so constituted that their controlling interests are not in the study of words, the forms of speech, or the boundless mass of information which is given in books." These appear to be his favourites. "The claims of this class of boys," he asserts, "have been set forth by no one so eloquently as by General Francis A. Walker." It may interest the reader to see what is regarded as surpassing eloquence by one who prescribes the limits of all literary education.

"It not infrequently happens that the boy who is regarded as dull because he cannot master an artificial